SCIENCE.

FRIDAY, APRIL 30, 1886.

COMMENT AND CRITICISM.

THE COMMISSION APPOINTED to investigate the various scientific bureaus of the government has submitted a partial report on the result of its labors, and has draughted a bill restricting the work and publications of the geological survey. Briefly, the measure provides, that, after June 30 next, no money shall be expended except for the collection, classification, and proper care of fossils and other material; no money is to be used for paleontological work or publications, nor for the general discussion of geological theories. The survey is to be prohibited from compiling or preparing for publication monographs or bulletins, or other books, except an annual report, which shall embrace only the transactions of bureaus for the year. All collections of minerals and other material now or hereafter to be made by the survey, and not needed for the current work thereof, are to be deposited in the national museum. The works whose publication is discontinued may be published by the authors at their own expense, who are to be allowed to copyright their material. The secretary of the interior is empowered to sell all the laboratories and other property now in use by the geological survey which shall no longer be needed after the passage of the proposed bill, and the proceeds of the sale are to be turned into the U. S. treasury. The bill provides that all printing and engraving done for the geological survey, coast and geodetic survey, and hydrographic office of the navy department, and the signal bureau, shall hereafter be estimated for separately, and prepared in detail for each of the said bureaus. The full report of the commission on the other bureaus is expected this week. The members claim that there has been great extravagance practised in the publication of works by the geological survey, and they propose to stop these 'reckless expenditures.' The report of the commissioners is unanimous in their action on the bill reported.

This report will be received with much regret by scientific men. The effect, so far as it pertains to the U. S. geological survey, should the bill become a law, will be most disastrous, crippling, if not almost entirely destroying, the survey's usefulness. Such sweeping and radical measures seem ill-advised. The causes that have led to the result, it is not hard to discover. Personal errors in other branches of the government surveys, and the exertions of a number personally opposed to the present management, will have placed the survey in a position from which it will be impossible to recover in many years. We do not need to repeat the argument, except to emphasize it, that national aid in the publication of many scientific works is absolutely necessary. In Europe such facilities exist in endowed scientific societies that do not exist in the United States, and will not for many years to come. The result simply will be that such works will not be published at all, and science will be so much the loser. Permission to copyright the works published at the expense of the author will only evoke a smile on the part of scientific men. One can imagine the danger likely to accrue to the author of a thousand-paged quarto on tertiary vertebrates, from his work being ruthlessly stolen, and issued in cheap paper form. The work of the geological survey has been managed honestly: no accusations whatever have been sustained against it. Neither can charges of extravagance in general be urged. The survey has perhaps grown to be too extensive; but the evil by no means calls for such severe pruning. Aside from arguments which will appeal to scientific men, it must be borne in mind that the survey can best justify its existence by furnishing valuable results to the miner and the farmer; and these results can only be reached when the evidence of all pertinent branches of investigation are available.

ABOUT A YEAR AGO much interest was taken in the discussion of requisitions for admission to colleges, when it was known that the faculty at Harvard had taken action in favor of recommending a sound course in laboratory study of chemistry or physics as an alternative for the admission requirements in Latin or Greek. A second step in this direction is now taken in the report of a committee of the board of Harvard overseers to

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that body, in which the following vote is recommended among others: "That, in the opinion of the board of overseers, it is advisable to permit a scientific substitute, in accordance with the terms of this report, to be offered by applicants for admission to the college for either Latin or Greek, one of these two languages always being required." The terms here referred to are substantially that the scientific substitute must be a real equivalent of the old language course in amount of time needed for it, and amount of training gained from it, and that this demands more than a 'textbook' and 'memory' study. The four members of the committee who present this majority report consider the scientific substitute above referred to as recommended by the college faculty an adequate one: a minority report from one member still maintains the need of Greek for all. Favorable action may therefore be expected from the overseers.

THE GREAT SUCCESS of the free lectures recently given at Columbia college by Professors Boyesen and Butler — applications for tickets to the second course numbering over two thousand — emphasize a point in university work that has been long and persistently overlooked; that is, the duty of the university toward the people at large. Our colleges and universities depend, for success and support, upon popular interest and encouragement. They are continually in want of money, and always desirous of attracting large numbers of students. A large endowment, provided it be judiciously administered, and a large body of students, constitute a successful university. Of course, the test of numbers is of itself of small value; but the college with a thousand students can create more enthusiasm, exert a wider influence, as well as find work for more instructors, than a college having only three hundred names on its roll. The test of numbers, then, stands not so much for itself as for what it implies and represents. But these two conditions of success — money and students — might be made much easier of attainment were the relations between the universities and the people closer than they now are. As a rule, the college professor is looked up to as a useless sort of individual, who knows a great deal, but whose knowledge is of a shadowy and unpractical character. Our professors are too prone to give encouragement to this opinion by shutting themselves up within the four walls of their studies and class-rooms, and producing no results of their labors that to the non-collegiate man seem practical. Persistence in this isolation must weaken the university, and cut it off from the very sources of its support. The university should have some message to the outside world that is of a less formal and abstruse character than that usually locked up in memoirs and the transactions of learned societies. For this the lecture-hall seems pre-eminently fitted, and through it can the university find that contact with the people that it so much needs. Especially in our large cities, and by the staff of instructors in our larger universities and colleges, is this plan feasible. For years the Johns Hopkins university has given courses of lectures on semi-popular subjects, and with great success; and now Columbia, in an informal sort of way, is trying the same experiment. Perhaps the great interest of the subjects of the courses that have already been given there — 'The tendencies of contemporary literature' and 'Education as a science' — have had much to do with the great success of the Columbia lectures; but we are fully convinced that a large variety of subjects, both literary and scientific, are capable of being treated by university professors in a way that will not only attract large audiences and be an educating influence among the people, but also bring life and strength to the university itself.

THE APRIL MEETING OF THE NATIONAL ACADEMY OF SCIENCES.

THIRTY-NINE members attended this year at the spring meeting of the academy, and found Washington in its most charming vernal dress. If we except the visit of courtesy made to the President of the United States, the only social incident of importance was a reception at which the members of the academy met the members of the local scientific societies for which Washington is justly celebrated.

The academy determined by vote not to consider the nominations that had been made for membership, so that no new members were chosen. The expiration of Professor Agassiz' term of office as foreign secretary created a vacancy; and, as he declined re-election on account of ill health, Prof. Wolcott Gibbs was selected to succeed him. Gen. M. C. Meigs and Profs. S. F. Baird, G. J. Brush, C. A. Young, E. C. Pickering, and S. P. Langley were elected to the council, and the remaining officers held over.

During the past year the government has made

three requisitions on the academy for information and advice. In accordance with a request of the secretary of the navy, a committee was appointed to consider, first, the question of the adoption of the universal day by the scientific bureaus of the department; second, the advisability of sending an expedition to observe the solar eclipse of August, 1886; and, third, the propriety of erecting a new naval observatory on the site selected in 1882. This committee submitted its report some months ago (*Science*, vii. 208). At the request of the treasury department, a committee was appointed to consider certain problems connected with the classification of wool for tariff purposes, and their report has become the basis of action by the department. More recently the treasury department has called on the academy for information affecting the subject of the duty on opium, and a committee has been appointed for this purpose.

The academy is now charged with the administration of three funds intended to stimulate astronomic research, and the trustees of these funds have decided to use portions of their incomes for suitable medals. The Henry Draper medal is given for researches in solar physics, the Lawrence Smith medal for studies of meteoric bodies, and the Watson medal for any distinguished achievement in astronomy. The first award of the Draper medal was made this year: and it was given to Prof. S. P. Langley, in recognition of the importance of his researches in solar physics. The Watson medal, with an honorarium of one hundred dollars, was awarded to Prof. B. A. Gould, in recognition of his distinguished service to astronomy in founding and conducting the Cordoba observatory.

A biographical notice of the late Prof. Arnold Guyot, prepared by Prof. J. D. Dana, was presented, and a similar notice of Prof. John W. Draper by Professor Barker. Professor Dana's memoir gave an account of Guyot's early life which will be new to many of his American friends, and particularly called attention to the fact that Guyot had made a scientific examination of the Alpine glaciers two years before they were studied by Agassiz, and anticipated a number of his most important conclusions. In a paper read then before the Helvetic society, but never printed until 1883, Guyot pointed out that the upper portion of the glacier moves faster than the lower, that the middle moves faster than the sides, that the general motion is accomplished by molecular motion, and he advanced the hypothesis that the blue bands are phenomena of the original stratification of the formative snow. Priority in these matters was not claimed by him, because, when he became soon afterward associated with Agassiz in glacial work, it was agreed that Agassiz' share should be the study of the living glaciers, and Guyot's the study of the erratic phenomena and other vestiges of ancient glaciation.

The only loss by death during the year has been that of Prof. Edward Tuckerman of Amherst, Mass. Prof. W. G. Farlow was selected to prepare a biographical notice.

The scientific proceedings of the academy occupied the afternoons of the four days of the session. Twenty-three papers were read and discussed, and four others were read by title. A list of the papers in addition to those announced last week will be found in another column. Here we have space to mention only a few.

Dr. A. Graham Bell reported the progress of his research regarding the ancestry of the deaf. Discovering from the statistics of asylums for deaf-mutes, and from the data of the tenth U. S. census, that deafness is exceptionally prevalent in Chilmark, in Martha's Vineyard, and in Kennebec county, Me., he visited those districts, and investigated the history of families affected. The deafness in Kennebec county is connected with that of Chilmark, and possibly derived from it. In both districts there is abundant evidence of heredity, and especially of atavism. In the families affected there were also found blindness, insanity, idiocy, and deformity; and in the Chilmark locality there has been such consanguineal marriage as is common to sedentary rural populations. The distribution of deafness on the island is closely related to that of soils. The affected families extend over the entire island; but the affected individuals are, with two exceptions, confined to a district of peculiar geological characteristics, and the eastern boundary of this district has been designated by local students of vital statistics as the typhoid-fever line.

By invitation, Mr. R. E. Peary, U. S. N., described his plans for an expedition to Greenland for exploration in the interior. He proposes to make a preliminary excursion from Disco Bay, and afterward an expedition from Whale Sound to some point on the east coast, near the 80th parallel. He prefers for the interior work a party of three, with snow-shoes, skiddars, and sleds modelled after the Hudson Bay pattern.

Prof. S. P. Langley reported the progress of his investigation of the invisible spectrum. Whereas Newton determined the indices of refraction of light-rays of wave-lengths ranging from .0003 to .0007 mm., Professor Langley has carried the determination to wave-lengths of .0400. He has also demonstrated a simple relation between wave-lengths and indices of refraction. The indices

of refraction being plotted as ordinates and the wave-lengths as abscissas, the resulting curve is found to be an hyperbola.

Prof. Alfred M. Mayer, in describing recent work, stated that he had succeeded, by the use of a lens of ebonite, in inflaming various substances by the concentration of dark rays, for which ebonite is translucent.

Dr. S. H. Scudder gave a general account of the cockroach in the past and present. Of all insect types, this one is best represented in the rocks, and especially the older rocks. The carboniferous, especially, may fitly be called the age of cockroaches. The paleozoic cockroaches were larger, the more recent smaller, than the modern. Mr. G. K. Gilbert discussed the geological age of the Equus fauna, maintaining that it belongs to the upper quaternary (later glacial), and not to the upper pliocene, where it had been assigned by students of vertebrate paleontology.

THE DATA NOW REQUISITE IN SOLAR INQUIRIES.

In order to obtain the greatest amount of assistance from observations of the eclipsed sun, it is necessary to consider in the most general way the condition of solar inquiry at the time the observations are made. If any special work commends itself to those interested in the problem, — work which may be likely to enable us to emphasize or reject existing ideas, — then that work should take precedence of all other.

Next, if the observers are sufficient in number to undertake other work besides this, then that work should be arranged in harmony with previous observations; that is, the old methods of work should be exactly followed, or they should be expanded so that a new series of observations may be begun in the light and in extension of the old ones.

In my opinion, and I only give it for what it is worth, the three burning questions at the present time — questions on which information is required in order that various forms of work may be undertaken to best advantage (besides eclipse-work) — are these: —

1. The true constitution of the atmosphere of the sun. By this I mean, are the various series of lines of the same element observed in sun-spots, e.g., limited to a certain stratum, each lower stratum being hotter, and therefore simpler in its spectrum, than the one overlying it? and do some of these strata, with their special spectra, exist high in the solar atmosphere, so that the Fraunhofer lines, represented in the spectrum of any one substance, are the result of an integration of the various absorptions from the highest stratum to the bottom one? This view is sharply opposed to the other, which affirms that the absorption of the Fraunhofer lines is due to one unique layer at the base of the atmosphere.

I pointed out before the eclipse of 1882 that crucial observations could be made during any eclipse, including the time both before and after totality. I made the observations: they entirely supported the first view, but I do not expect solar inquirers to throw overboard their own views until these observations of mine are confirmed; and I think one of the most important pieces of work to be done during the next eclipse is to see whether these observations can be depended upon or not.

One observer, I think, should repeat the work over the same limited region of the spectrum, near F; another observer should be told off to make similar observations in another part of the spectrum. I have prepared a map of the lines near E, for this purpose, showing those brightened on the passage from the arc to the spark, and those visible alone at the temperature of the oxyhydrogen flame. Whereas some of the spark lines will be seen seven minutes before and after totality as short, bright lines, some of the others will be seen as thin, long lines just before and after totality. We want to know whether the lines seen at the temperature of the oxyhydrogen flame will be seen at all, and, if so, to what height they extend.

2. The second point to which I attach importance is one which can perhaps be left to a large extent to local observers, if the proper apparatus, which may cost very little, be taken out.

With this eclipse in view, I have for the last several months gone over all the recorded information, and have discussed the photographs taken at the various eclipses in connection with the spots observed, especially at those times.

The simple corona observed at a minimum with a considerable equatorial extension (12 diameters, according to Langley), the complex corona observed at maximum when the spots have been located at latitudes less than 20°, have driven me to the view, which I shall expand on another occasion, that there is a flattened ring round the sun's equator, probably extending far beyond the true atmosphere; that in this ring are collected the products of condensation; and that it is from the surfaces of this ring chiefly that the fall of spot-forming material takes place.

If we take any streamer in mid-latitude, we find, that, while the spots may occur on the equatorial side of it, none are seen on the poleward side. I regard the streamers, therefore, like

the metallic prominences, as a sequel to the spot; and there is evidence to suggest that a careful study will enable us to see by what process the reaction of the photosphere and underlying gases produced by the fall of spot-material tends to make the spot-material discharge itself in lower and lower latitudes, as the temperature of the sun's lower atmosphere gets enormously increased.

The observations of Professors Newcomb and Langley at the minimum of 1878, on the equatorial extension, are among the most remarkable. Professor Newcomb hid the moon and 12′ of arc around it at the moment of totality by a disk of wood, carefully shielding his eyes before totality. Professor Langley observed at a very considerable elevation. It is therefore quite easy to understand why this ring has not been seen or photographed at maximum. At maximum no precautions have been taken to shield the eye; no observations have been made at a considerable elevation; while the fact that the ring, if it exists, consists of cool material, fully explains how it is that the photographic plates have disregarded it.

I would propose, therefore, that the repetition of Professor Newcomb's observations of 1878 be made an important part in the arrangements of the eclipse for this year. A slight alteration in the method will be necessary, as the ring will be near the vertex and the lowest point of the eclipsed sun.

3. Another point of the highest importance at the present moment has relation to the existence of carbon. Until Tacchini's observations of 1883, the only trace of carbon in the solar spectrum consisted of ultra-violet flutings. He observed other flutings in the green near the streamers in the eclipse referred to.

Duner's recent work puts it beyond all doubt that stars of class III. *b* have their visible absorption produced chiefly by carbon vapor.

On any theory of evolution, therefore, we must expect the sun's atmosphere to be composed to a large extent of carbon at some time or other; so that the highest interest attaches to this question in connection with the height in the atmosphere at which the evidence of carbon is observed. The existence of the ultra-violet flutings among the Fraunhofer lines tells nothing absolute about this height, although I inferred, at the time I made the announcement, that it existed at some height in the coronal atmosphere.

These three points, then, are those to which I attach special importance at the present time.

We next come to photographs of the corona. I believe, that, with our present knowledge, the chief thing we have to seek in such photographs is not merely the streamers and their outlines, which we are sure to get anyway, but images on a larger scale; so that in a series of short exposures we may endeavor to get some records which will eventually help us in determining the directions of the lower currents. At present we do not know absolutely whether these flow to or from the poles. My own impression is that the panaches at the poles indicate an upper outflow.

In coming to the photo-spectroscopic observations, I am of opinion, that of the two attacks which I first suggested for the eclipse of 1875, and which have also been used in the last two eclipses of 1882 and 1883, one of them should be discarded, and the whole effort concentrated on the other.

We have learned very much from the use of the prismatic camera, — one of the instruments referred to; but the results obtained by it are not of sufficient accuracy to enable them to be fully utilized. On the other hand, though the slit spectroscope failed in 1875, it succeeded with a brighter corona and more rapid plates in 1882; and, with a proper reference spectrum, every iota of the facts recorded can be at once utilized for laboratory work and subsequent discussion.

On these grounds, then, I would suggest that slit spectroscopes alone be used for photographic registration. I think falling plates should be used, and that the work should begin ten minutes before totality, and continue till ten minutes after; provided the slit be tangential, or nearly so, to the limb.

I may state that arrangements have been made here to take such a series of photographs on the uneclipsed sun; and, with the improved apparatus, I am greatly in hopes that we may get something worth having. J. NORMAN LOCKYER.

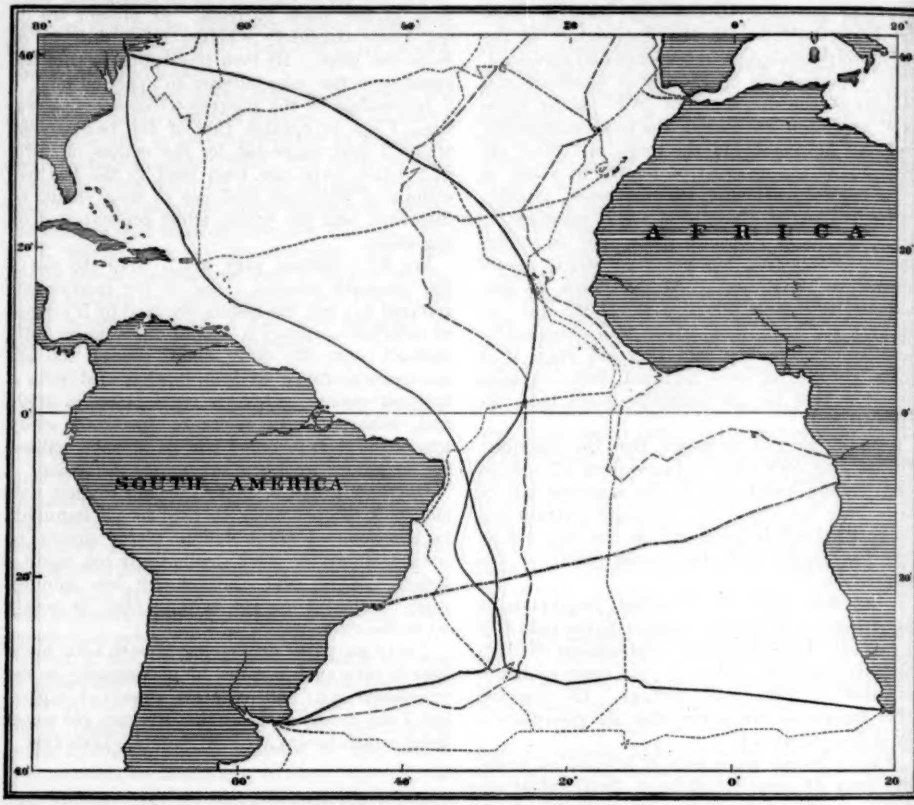
DEEP-SEA SOUNDINGS IN THE ATLANTIC.

THE U. S. S. Enterprise, Commander Barker, during her recent passage from Montevideo to Barbadoes, and from thence to New York, made a series of deep-sea soundings through the Atlantic Oceans which add considerably to our knowledge of the depths of those seas. Seventy-two casts were taken between Montevideo and Barbadoes, the distance run being 5,031 miles.

After leaving Montevideo, the course of the Enterprise was laid to the northward, towards Nelson shoal, where a depth of 2,088 fathoms of water was found, instead of 19 fathoms, as appears on all the charts of that locality. Commander Barker says, "From this point I steamed slowly, running from 200 to 250 miles to the northward of the Challenger's line, taking casts at in-

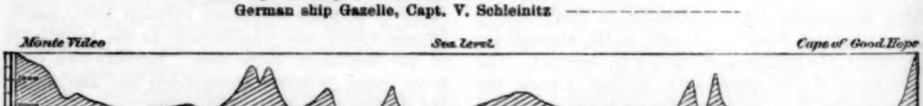
tervals of about sixty miles, the average depth being about 2,000 fathoms. In latitude 31° 22′ south, longitude 36° 39′ west, the water shoaled to 1,469 fathoms; and the next cast, taken in latitude found was 378 fathoms, in latitude 31° 02′ south, longitude 34° 27′ west."

This bank, which it is proposed to call Enterprise bank, extends about 150 miles in longitude.



DEEP-SEA SOUNDINGS IN THE ATLANTIC.

U.S.S. Enterprise, Commander A. S. Barker
U.S.S. Essex, Commander W. S. Schley
British ship Challenger, Commander G. S. Nares
German ship Gazelle, Capt. V. Schleinitz



PROFILE OF OCEAN-BED BETWEEN MONTEVIDEO AND THE CAPE OF GOOD HOPE AS SHOWN BY THE DEEP-SEA SOUNDINGS OF THE U.S.S. ENTERPRISE.

31° 15′ south, longitude 35° 42′ west, was only 547 fathoms. From this position casts were taken at intervals of five miles or thereabouts until over the shoalest part of the bank. The least depth It may be much shoaler in other places than those sounded over, as its extent in latitude is not known, and there have been no soundings in that neighborhood which will admit of any generaliza-

tions in regard to it. The hydrographic office will have it further examined at the first opportunity.

From this point the easterly course was continued until the line of soundings taken three years before by the Enterprise was crossed, in about latitude 27° south, longitude 27° west; and then the line ran almost directly for the Island of Fernando de Noronha, the depths averaging about 2,800 fathoms, until the vicinity of this island was shown by a sounding of 2,280 fathoms. Beyond, the depths increased to an average of about 2,500 fathoms until the neighborhood of Barbadoes was reached, when the water shoaled again to 1,204 fathoms.

The depth of 2,560 fathoms in longitude 55° west, latitude 12° north, is within thirty miles of a sounding of 2,570 fathoms taken by the U.S. brig Dolphin in 1852; that of 2,714 fathoms in latitude 11° 25′ north, longitude 52° 50′ west, is within thirty miles of a sounding of 2,780 fathoms, also taken by the Dolphin in 1852.

After leaving St. Thomas, sounding was again resumed; the first cast, taken in latitude 19° 53′ north, longitude 65° 45′ west, showing 4,529 fathoms. As this point is about forty miles east-north-east of the famous cast of 4,561 fathoms, made by Lieutenant-Commander Brownson, U.S.N., with the coast and geodetic survey steamer Blake, the great depth obtained is peculiarly interesting. Beyond this deep the line ran towards Cape Hatteras, over a section formerly unsounded, showing an average depth of about 3,000 fathoms.

Commander Barker further says, "A ship like the Enterprise can undoubtedly sound in any sea and in any weather in which she can steam ahead fast enough to stem the wind and steer. The brake used was a plain piece of rope made fast inboard of, and abreast of, the lower part of the reel, then around the groove outboard, and held in the hand above. This brake controls the reel perfectly, it being possible to hold the shot, without any effort, at a great depth. In rolling heavily it is very easy to keep a constant strain on the wire. A distance-line of at least 12 fathoms was used, with a piece of lead weighing about a pound near the grommet. One length of the large American wire was put on next to the distance-line, as it was not so likely to kink. To prevent the shot from catching on top of the cup, a tripping-line was used, consisting of a piece of small stuff, one end made fast to the rod just below and in the plane of the hook, and the other end around the top of the cup: this line is of such a length as to be taut when the cup is closed. In nearly all the casts, sail was made after reeling in to 2,000 fathoms, but only such as not to give a greater speed than four knots. When reeled in to 1,000 fathoms, all sail was made. The wind was always kept on the starboard side, so as to have the wire to windward. The only accident which happened on the trip was due to the wire catching some part of the ship, probably the propeller: it was dark at the time, and she was going at the rate of about seven knots." The accompanying chart shows the principal lines of deep-sea soundings south of latitude 40° north. The hydrographic office has in course of preparation a series of charts showing the contours of the ocean-beds as determined by all reliable soundings that have been taken.

J. R. Bartlett.

U. S. hydrographic office.

LONDON LETTER.

After more than seven years of investigation and experiment, the Royal commission appointed to inquire into accidents in mines has presented its final report, which was issued on Saturday in the form of one hundred and ten pages of a large blue-book. The delay is accounted for by the long and difficult quest on which the commissioners were sent. They were to report, not only on the causes of mining accidents, but also on "the possible means of preventing their recurrence, or limiting their disastrous consequences." Not much is recommended in the way of mere legislative changes, but the scientific recommendations are most interesting and important. For example: with reference to the difficult question of the best method of firing shots in mines, they state that "electrical exploding appliances present very important advantages from the point of view of safety, over any kind of fuze which has to be ignited by the application of flame to its exposed extremity, as the firing of shots by their means is not only accomplished out of contact with air, but is also under most complete control up to the moment of firing. Their simplicity and certainty of action has been much increased of late years, while their cost has been greatly reduced, and but little instruction is now needed to insure their efficient employment by persons of average intelligence. The use of electrical arrangements for firing shots in mines where the employment of powder for blasting is inadmissible should be encouraged as much as possible."

Again, they state that "it has been shown that mines which have hitherto been considered free from fire-damp may have the air which passes through them vitiated to an extent corresponding to about two per cent of its volume of marsh-gas. The air in many such mines may probably never be entirely free from explosive gas; at all events, in the neighborhood of freshly cut faces of coal

and in the return air-ways. It has been demonstrated in our experiments, that, when the atmosphere contains five to five and one-half per cent of marsh-gas, it becomes highly explosive. We have even obtained explosions which, though less violent, might be nevertheless destructive of life if they occurred, on the large scale possible in a mine, when the air contained only four per cent of marsh-gas. It will thus be seen that air which would appear free from gas if tested in the ordinary way, may become, by the addition of only about two per cent of marsh-gas, capable of propagating flame and causing destruction, while the addition of about three per cent converts it into a highly explosive mixture. Air which would appear quite free from gas if examined by a lamp-flame, may become explosive when laden with fine, dry coal-dust. Appliances now exist by which very small proportions of marsh-gas in air may be readily detected, and which can be used for examining the atmosphere of a mine. With Liveing's indicator, gas present in the air can be estimated with sufficient accuracy for all practical purposes, even when the proportion is as low as one-quarter per cent."

In connection with this subject, the suggestion, first due to Mr. Galloway, that coal-dust alone suspended in air might cause an explosion, is considered, and an account is given of some carefully devised experiments which rather tend to confirm this conclusion. The commissioners discuss with some detail the means of removing this dust, and devote a large section of the report to the question of the conditions under which blasting can be done in safety. Considerable space is devoted to safety-lamps, and it is pointed out how great an influence the velocity of the air-currents in the air-passages of a mine has on the safety of a lamp. The electric lamp is perhaps the chief hope of the miner, though it does not, like the safety-lamp, indicate the presence of gas. The commissioners arrived at the following conclusions: "that it is most important that all mines should be carefully examined by means of indicators capable of detecting as small a proportion as one per cent of gas; such examination to be made before the commencement of each day-shift, and, in case of an interval, also before the succeeding shift; and that in all dry mines where the air may be laden with coal-dust, and where fire-damp is either known to be given off from the strata, or may from experience be reasonably suspected to exist, the secretary of state may require safety-lamps to be used, unless the owners and workmen of such mines prove to the satisfaction of a court of arbitration, to be appointed by the respective parties, that less liability to accident generally will be involved by the working of the mine with open lights than by use of safety-lamps. It should be a special instruction to such court that the circumstances of each mine be taken into consideration."

The late Prof. John Morris, who died in January last, had been engaged for some time in preparing a third edition of his invaluable 'Catalogue of British fossils.' The first edition was published in 1843, and the second in 1854. From that date onwards, Professor Morris had been collecting materials for a third edition, which, unfortunately, he did not live to complete. But his manuscripts have been placed in the hands of a committee, which includes the keeper of the geological department in the Natural history museum, the president of the Geological society, and other well-known geologists. They have divided up the work among several specialists, who have engaged to finish their respective parts within six months; and it is therefore hoped that this great work may be completed before very long.

The publication of the Challenger volumes is now proceeding rapidly. No less than fourteen reports are at present passing through the press, and it is expected that the entire series will be completed by the end of next March.

The Lumleian lectures, now in course of delivery before the College of physicians by Dr. W. H. Stone, are attracting unusually large audiences. Their subject is 'The electrical conditions of the human body.' Dr. Stone was one of the first to call attention to the importance of determining accurately the physical constants of the agent electricity when employed in physiological investigation. In these lectures he has shown that most of the contradictory results obtained by the earlier investigators are due to the neglect of this precaution. The enormously high resistance of the epidermis was demonstrated; and, when this was eliminated, the average resistance to a continuous current from the ulna at the wrist to the malleolus at the ankle, was shown to be about 1,170 ohms, due allowance being made for the errors caused by polarization, according to the ingenious method first devised by Sir Henry Mance for the Persian Gulf cables. Some entirely new experiments were detailed, and in part repeated before the audience, showing that the human body could be charged and discharged like a secondary battery. An electromotive force of two volts was employed, and curves showing the rate of discharge were exhibited. A discharge current of sixty micro-ampères at first, under an electromotive force of about one volt,

sank to forty-eight in five minutes, and remained at that for some hours. The resistance offered by the body to an induced current was stated to be only half that offered to a continuous one. An ingenious speculation was hazarded as to the possibility of the human nervous system distantly resembling a duplexed telegraph-cable, in which a transmitted impulse is balanced and inhibited at the sending-station, but unbalanced and exhibited at the receiving-station. W.

London, April 13.

NOTES AND NEWS.

THE following, in addition to those given in our last issue, completes the list of papers read at the National academy of sciences, April 20–23: Alfred M. Mayer, On the diathermancy of ebonite and obsidian, and on the production of calorescence by means of screens of ebonite and obsidian; On the coefficient of expansion of ebonite; On the determination of the cubical expansion of a solid by a method which does not require calibration of vessels, weighings, or linear measure; On measures of absolute radiation; E. D. Cope, On the geology of the region near Zacualtipan, Hidalgo, Mexico; Edward S. Morse, On ancient and modern methods of arrow release; Theo. Gill, The ordinal and super-ordinal groups of fishes; H. A. Rowland, On the absolute and relative wave-lengths of the lines of the solar spectrum; Wolcott Gibbs, Platinous compounds as additive molecules; Ira Remsen, Influence of magnetism on chemical action; A. Graham Bell, Upon the deaf and dumb of Martha's Vineyard (continuation of research relating to the ancestry of the deaf); S. P. Langley, On the invisible spectra; G. F. Becker, Cretaceous metamorphic rocks of California (by invitation); Ogden N. Rood, On color contrast; Charles D. Walcott, Classification of the Cambrian system of North America (by invitation); A. W. Wright, Crystallization of platinum by means of the electric discharge *in vacuo;* W. K. Brooks, The Stomatopoda of the Challenger collection; Budding in the Tunicata; A. W. Wright, Effect of magnetization on the electrical resistance of metals; R. E. Peary, U.S.N., On a proposed expedition into the interior of Greenland.

LETTERS TO THE EDITOR.

Science at Cornell.

MY attention has been called to the communication signed 'H. N.' in *Science* for April 16, and I beg for a little space in which to point out one or two errors into which the writer has fallen.

I shall not attempt to deal with the swarming misstatements and exaggerations of the letter. These, although inviting game, are comparatively unimportant. But the fundamental idea of the writer is not without importance, and therefore should not pass unnoticed. That idea is divisible into two parts. The first is, that Cornell university, in developing its non-technical side, is doing violence to the fundamental law and charter of the institution: and the second is, that, in so doing, 'the successor of Andrew D. White' is reversing the traditions and former policy of the university. "Where," exclaims the writer, "are the traditions and the law and charter of Cornell?" Let us see.

First, The fundamental law declares its purpose in the words, "in order to promote the liberal and practical education of the industrial classes in the several pursuits and professions of life." To accomplish this declared purpose, which, it will be seen, is of the broadest possible character, the law required "the endowment, support, and maintenance of at least one college where the leading object shall be, without excluding other scientific and classical studies, and including military tactics, to teach such branches of learning as are related to agriculture and the mechanic arts." How this shall be done is explained in the clause, "in such manner as the legislatures of the states shall respectively prescribe."

Here we see, in the language of the law itself, a purpose that is clearly unmistakable. It includes not simply agriculture and the mechanic arts, but 'other scientific and classical studies,' 'military tactics,' and 'the several pursuits and professions of life.' Furthermore, these provisions shall be carried out in such a way as the legislatures of the states may severally prescribe. So much for the fundamental law.

Second, The charter of the university, after repeating the provisions of the fundamental law, and doubtless in view of the very large gift of Mr. Cornell, adds the following sentence: "But such other branches of science and knowledge may be embraced in the plan of instruction and investigation, pertaining to the university, *as the trustees may deem useful and proper.*" In other words, the trustees are left by the charter to determine precisely what branches of science and knowledge shall be embraced in the plan of instruction, after those specifically provided for have been established and duly equipped.

Third, Now as to traditions. As soon as the trustees named in the charter came together, the first thing to be done was to determine upon a plan of organization. A committee for that purpose was appointed, of which Andrew D. White was chairman. On the 21st of October, 1866, he presented his famous report. In the very first part of it, under the head of 'Fundamental plan of instruction,' he argues the very question which lies at the bottom of 'H. N.'s' grievance. He is of opinion that the fundamental law justifies the establishment of all the departments of a true university. But, even if it did not, he finds unmistakable warrant in the provisions of the charter. In order that there may be no possible misunderstanding of President White's views, I quote a single sentence from p. 4 of the report: "Even if it should be claimed that the whole effort of the trustees ought to be devoted to agriculture and the mechanic arts alone; even if we were to construe away the plain words of the original act of congress, which speaks of 'other scientific and classical branches' as part of the object of the government grant of lands, — still the oft-repeated declaration of our founder, that he 'wishes to make such provision that every person can find opportunity here to pursue any study he desires,' would be our

sufficient warrant in using at least his munificent gift in supplementing the special instruction with general instruction, and rounding it out into the proportions of a university."

Now, proceeding on this theory, under the head of 'Organization,' President White gives a list of the departments which he thinks ought to be established. Conspicuous in this list, on p. 5 of the report, is the department of medicine and surgery, and the department of law. Then on p. 13 of the same report I find, in the list of professors, the appointment of whom he recommends, — a 'professor of municipal law,' and a 'professor of constitutional law.' For the purposes of this presentation it is, of course, needless to speak of the other departments contemplated in the plan of organization.

Now, I have read all the speeches, and I believe all the reports, of President White; and I believe there is not a passage in one of them, from first to last, that contradicts, either in letter or in spirit, the doctrine here set forth. I will go further, and say that through them all is to be seen the same spirit as that manifested in the 'plan of organization.' This is my answer to 'H. N.'s' grandiloquent inquiry, "Where are the traditions and the law and charter of Cornell?"

It has never been claimed, and is not now claimed, that the technical departments are of secondary importance; but, as I asserted in my address at New York, I hold that these departments have now so far been provided for, that the time has arrived when attention should be called to the needs of other departments. I do not mean by this that the university is to cease its appropriations for the technical schools. So far as I know, it has no such intention. This, indeed, may fairly be inferred from the fact that at the present moment the trustees are taking steps for the immediate erection of an ample building for the veterinary department, and to add four rooms to the agricultural museum. We shall do still more in the same direction, but it is not the purpose of the trustees to limit the activities of the university to a single one of those interests, comtemplated at the time of its organization, and, indeed, throughout its history.

No revolution is taking place at Cornell. On the contrary, its trustees are trying to develop it strictly along the line of its fundamental law, its charter, and its traditions. Surely it is late in the day for this university to be turned from such a purpose by any hint that its charter is in danger.

C. K. ADAMS.

Cornell university, April 26.

Popular astronomy.

I think the author of the article 'Popular astronomy' (*Science*, April 23), in his chivalric defence of the rights of Professor Newcomb and myself, has really done a serious injustice to Dr. Ball in virtually charging him with deliberate plagiarism and "a continued effort to conceal the theft, which is petty in the extreme," by slight alterations of the borrowed material. No one personally acquainted with Dr. Ball could possibly suspect him of intentional wrong in the case: I believe him to be totally incapable of any thing dishonorable.

Judging from my own experience, which, though not extensive, has been exactly to the point, a very simple explanation will account for the apparent appropriation of other people's language, which is the foundation of the charge. In preparing for lectures to college classes and to popular audiences, I collect all the material I can find, and, in speaking, use it liberally. Of course, I indicate in a general way my obligations and sources of information; but it is quite impossible, while speaking, to point out every place where I am using language suggested by my reading. In fact, not having the matter written out, it is not possible (for me at least) to quote accurately the words of my authority; and, after a few repetitions of the lecture, the *quasi* quotations become modified by changes that make them conform to my usual forms of expression, and render them, so far as consciousness is concerned, quite as much my own as any other part of the lecture. At the same time they would be quite recognizable by one familiar with the original.

Now, in making a book upon the subject upon which one has been lecturing, he will inevitably write pretty nearly what he would say if standing before an audience, and in this way will quote, unconsciously and more or less inaccurately, passages of considerable length from the works he used in his original lecture-preparation. The only way I know of to do justice in the matter, is first to put into the preface of the book a full general acknowledgment of obligations, and then to go over the manuscript, lecture-notes in hand, hunting up and marking all these unconscious quotations, and restoring them to their original form.

Dr. Ball seems to have failed in doing this thoroughly, and hence, no doubt, the oversights which have led to the charge of guiltily disguised plagiarism. I am sure he meant no wrong, and I am greatly complimented and flattered by his approval and use of my work.

C. A. YOUNG.

Princeton, N.J., April 24.

As Sir Robert Ball is on the other side of the Atlantic, I deem it proper to say that he has satisfactorily explained the circumstances alluded to in the last number of *Science*. Although this explanation only refers to the copying of passages from my 'Popular astronomy,' I have no doubt that his remarks would apply equally to the close parallelism of passages in his book, and in Professor Young's treatise on the sun. His statement is as follows: —

"Your sketch of the discovery of the companion of Sirius I transcribed some years ago, before I had any thoughts of writing my book. The passage about Tycho I had, however, more recently taken. When I came to prepare the materials for the press, I lost sight, it seems, of the source of these passages, and treated them as if the language had been my own.

"Not until yesterday, when I read the review in the New York *Nation*, did I know that my book contained any passage virtually yours, except that duly acknowledged on p. 231."

I suppose this is an inadvertence of which any of us might be guilty who are in the habit of copying passages for use in popular lectures, or as memoranda for any other purpose.

S. NEWCOMB.

Arsenic in wall paper.

A note in *Science* (April 23, p. 371) says, "The investigation before the Massachusetts legislative committee on the subject of arsenic in wall-paper indi-

cates that the danger has been exaggerated." So far is this from being the case, and so great is the real danger, that I beg space for the presentation of some facts. The immediate cause of the present investigation was a letter published in the Boston *Herald* on Jan. 19, in which I gave a detailed account of sufferings in our own house due to arsenic in the wall-papers, and involving all the members of the household. Since that time many persons have published similar accounts in the Boston papers. Abstracts of twenty-two such letters appeared in the Boston *Advertiser* of March 2 and 12, fourteen of the same appearing in the Boston *Herald* of March 2; and in the four hearings given by the public health committee to the petitioners a mass of evidence was presented which must have convinced any unprejudiced mind. The committee have not yet made their report to the legislature, but it is expected that they will soon do so. The statement has already been published in the Boston papers, that the committee will recommend legislation, and it would be a matter of great surprise if they should do otherwise, — a surprise even to those who are trying to defeat legislation.

Science also adds, "Prof. C. F. Chandler testified, that, from careful experiments, under no conditions could arsenical poisoning occur through breathing arseniuretted hydrogen from wall-paper, and that the only source of danger would be from friction alone." In point of fact, Professor Chandler's testimony was much stronger than this. He not only stated that he believed the generation of arseniuretted hydrogen from arsenical wall-papers to be impossible, but he also said of this gas that he considered 'a small quantity comparatively harmless.' As to the legislation, for which those of us who have suffered were asking, he said that he was 'not in favor of any law on the subject;' that personally he was 'not afraid of arsenical wall-paper under any circumstances, with any quantity;' and that he considered the evidence of persons who suppose that they have suffered from wall-paper poison to be 'of very little value.' He also said that some years ago he investigated the whole subject of dangers from arsenical wall-papers, 'and concluded that there was nothing in it;' while his conviction that the generation of arseniuretted hydrogen from arsenical wall-papers is impossible was based on experiments made by two of his students in his laboratory six years ago.

As to all the essential points involved in the investigation, the petition is supported by the best chemical opinion in Harvard university, by some of the best medical opinion in Massachusetts, and by a body of evidence from actual sufferers unimpeachable and unanswerable. But I desire specially to call attention to the fact that Professor Chandler himself gives indirect support to the petition. As one of the original editors of Johnson's 'Universal cyclopaedia,' and one of the active editors in the revision now going through the press, Professor Chandler publishes in vol. i. (New York, 1886) an article on arsenious oxide, wherein he calls attention to the danger from arsenical paper. His language is, "Recent inquiry would lead to the belief that rooms covered with paper coated with this green arsenite of copper are detrimental to health, from the readiness with which minute particles of the poisonous pigment are detached from the walls by the slightest friction, are diffused through the room, and ultimately pass into the animal system. It is also said that arseniuretted hydrogen (H_3As), a very poisonous gas, is generated in damp weather."

True, this language was first written for an earlier edition; but inasmuch as no expense was spared in the revision (see publisher's announcement), and inasmuch as Professor Chandler was one of the revisers, the language may be taken as the utterance of all that Professor Chandler considered it worth while to say at the time when the new volume was published. I have called this article an '*indirect* support' to our petition, because, although the writer does not squarely state an opinion of his own, yet his language undoubtedly makes the impression that he considers the subject an important one,—one, indeed, which he has not investigated, and on which he therefore has not formed an opinion, but important enough to call attention to the danger.

It is also interesting to observe that one of the authorities whom Professor Chandler quotes against the theory that arseniuretted hydrogen escapes from arsenical wall-papers has subsequently changed his opinion. I refer to Watts's 'Dictionary of chemistry.' So far as I have been able to learn, the last expression of Dr. Watts on the subject in hand is found in the third supplement, which is vol. viii. of the whole work, in part i. p. 122 (London, 1879). There we read, "*Arsenic in the air of rooms.* — From experiments by H. Fleck (*Zeitschr für biologie*, viii. 444), it appears that the air of rooms, the carpets or wall-papers of which are colored with Schweinfurth green, often contains arseniuretted hydrogen, produced by the action of moisture and organic matter on the arsenical pigment. The size, starch, paste, etc., used in hanging the paper, appear to be especially active in this respect."

Also another authority, whose opinion of 1862 Professor Chandler quotes against our petition, has long since given up that opinion. I refer to Dr. Hoffman of Berlin. Dr. Hoffman was one of the scientific men summoned a few years ago to aid the German royal sanitary commission in investigating the dangers from arsenic in objects of domestic use. Dr. Hoffman's present opinion is seen in the report of the commission, which resulted in a stringent law in Germany. The language bearing on this subject is as follows: "Wall-papers are deserving special attention, and also window-curtains, which frequently contain large amounts of arsenic. The injurious action of this is not only through the lading of the atmosphere with arsenical dust, but also from the continued formation of arseniuretted hydrogen, a gas extremely dangerous to health."

I am happy to state that the public health committee of the Massachusetts legislature have ordered the publication of the stenographic report of the hearings given on this subject, and this document cannot fail to be of value to the legislative committees of other states or of congress when the enormity of the arsenic evil shall become more widely known.

D. G. LYON.

Cambridge, Mass., April 24.

On two plates of stratigraphical sections of the Taconic ranges by Prof. James Hall.

In an article in the number for April, 1886, of *The American journal of science*, entitled 'On lower Silurian fossils from a limestone of the original Taconic of Emmons,' on p. 247, the author speaks of

a 'most welcome addition,' to the stratigraphy of the Taconic range, of two plates of stratigraphical sections' by Professor Hall, 'prepared by him forty to forty-five years since.'

Those two plates, or rather five plates, for that is their exact number, were freely distributed by Professor Hall as far back as Lyell's second visit to America, 1845-46, and are well known on both sides of the Atlantic.

Professor Emmons refers to them in one of his letters, dated Raleigh, N.C., Dec. 28, 1860, of which I published an extract in 'The Taconic system and its position in stratigraphic geology' (*Proc. Amer. acad. arts and sciences*, vol. xii. p. 128, Cambridge, 1885), as follows: "You are aware that [Professor] Hall prepared five long sheets of sections illustrating his views, and which extended from the Helderberg to the Connecticut River, and from the Lake Champlain to the Connecticut valley. . . . They were designed to sustain his peculiar views. I have copies, and I wish you had them. They are curiosities in their way."

It is evident that the views entertained by Professor Hall, contesting the conclusions of Dr. Emmons, have been placed before geologists in the United States, Canada, and Europe since the appearance of 'The Taconic system' in 1842.

JULES MARCOU.

Cambridge, Mass., April 23.

A carnivorous butterfly larva.

One of the most interesting of our butterflies is that known as Fenesica tarquinius, — a unique lycinid having the wings above brown-black in color, with conspicuous orange markings both on primaries and secondaries. It has a wide geographical range, occurring very generally over North America, as also in Asia.

Donovan, in his 'Insects of India' (pl. xliv. fig. 1), illustrates the butterfly rather poorly, but says nothing about the larva; Boisduval and LeConte (*Hist. des lep. et des chen. de l'Amer. Sept.*, p. 128, pl. 37) figure the larva, pupa, and imago under the name of Polyommatus crataegi, and simply quote Abbot as stating that the larva lives in several species of Crataegus; Scudder (*Proc. Essex inst.*, iii. p. 163, 1862) treats of it under the name of Polyommatus porsenna (*Syn. list of Amer. rurales*, Bull. Buff. soc. nat. hist., iii. p. 129, May, 1876), giving the food-plants of the larva as Alnus, Ribesia, Vaccinium, and Viburnum (later, in the *American naturalist* for August, 1869, he gives the food-plants as follows, — 'probably arrow-wood, elder, and hawthorn'); Grote (*Trans. Amer. ent. soc.*, ii. p. 307) first proposed the generic name of Fenesica, but says nothing about its larval history; Strecker (*Butt. and moths*, etc. — Diurnes, p. 103) repeats simply from Scudder; while William H. Edwards, in his admirable life-histories of butterflies, has not, so far, treated of this particular species. In short, so far as the published records go, it has been generally assumed that the larva feeds upon the plants named.

The object of this brief communication is to show that in this larva we have one that is truly carnivorous,—a fact which is extremely interesting, because, so far as I can find, there is not another recorded carnivorous butterfly larva; and Mr. Scudder, who has given great attention to the butterflies, writes me in a recent letter, in reply to an inquiry on this point, that he cannot recall any mention of such. Quite a number of heterocerous larvae are known to be carnivorous by exception, and not a few are so as a rule. These are chiefly found among pyralids; and it is not necessary, for my present purpose, to refer to the cases in detail.

For some years, now, I have been studying the remarkable life-habits of the Aphididae, and especially of some of the gall-making and leaf-curling species of Pemphiginae.

In collecting material and making observations, I have been assisted by Mr. Th. Pergande, who has, on a number of occasions since 1880, found the larva of this Fenesica associated with various plant-lice. Among the species with which it has been thus found associated are Pemphigus fraxinifolii Riley, which curls the leaves of Fraxinus; Schizoneura tessellata Fitch, which crowds upon the branches of Alnus; and Pemphigus imbricator Fitch, which congregates in large masses on Fagus. All these species produce much flocculent and saccharine matter.

The frequency with which this larva was found among these plant-lice justified the suspicion that it feeds upon them or derives benefit from them; yet up to 1885 the presumption was that it benefited from the secretions of the plant-lice rather than from the insects themselves. Last fall, however, Mr. Pergande obtained abundant evidence that the Fenesica larva actually feeds upon the aphidids, and I thought it worth while to call attention to this positive proof of the carnivorous habits of the species. That the different species of plant-lice are the normal food of this larva, is rendered more than probable for the following reasons: —

1. Attempts to feed the larva upon the leaves upon which it was found have proved futile, the larva perishing rather than feed upon them.
2. The food-plants given by the authorities are such as are well known to harbor plant-lice.
3. Mr. Scudder's authorities, as he informs me, were picked up here and there; and one of them for alder, which he recalls, 'found it more commonly on a limb among plant-lice.'
4. Mr. Otto Lugger has frequently observed the larva around Baltimore among Pemphigus imbricator on beech, but never disassociated from the lice; and Judge Lawrence Johnson also found it in connection with the same species around Shreveport, La., last fall, and surmised that it might feed upon the Pemphigus; but neither of these observers were able to get positive proof of the fact.

C. V. RILEY.

Combined aerial and aquatic respiration.

In investigating combined aerial and aquatic respiration in vertebrates, the following questions have presented themselves for solution, — questions which, so far as we have been able to ascertain, have not been previously answered by physiologists: —

1. Is the aerial part of the respiration like that of animals with an exclusively aerial respiration?
2. Is the aquatic part of the respiration like that of animals with an exclusively aquatic respiration?

In answer to these questions, we offer the following facts and conclusion: —

1. Observations upon the aquatic respiration of soft-shelled turtles (*Science*, vi. p. 255; and *Amer. nat.*, 1886, p. 233) showed that the air taken from the lungs of a turtle that had been immersed several hours, had been almost completely deprived of its

oxygen, while but a trace of carbon dioxide had been added to it. The water in which it had been immersed had received, however, a much greater amount of carbon dioxide than could have been formed from the free oxygen taken from the water.

2. Tadpoles were placed in a jar partly filled with water, and the jar hermetically closed. After several hours, the air was analyzed, and the free gases in the water determined. These determinations showed that nine tenths of the oxygen consumed came from the air, and one tenth from the water; while, of the carbon dioxide produced during the experiment, the air contained three tenths, and the water seven tenths.

In order that the carbon dioxide given off by the tadpoles to the air might not be absorbed by the water during the experiment, a layer of olive-oil six millimetres thick was put upon the water.

3. It was found by careful and repeated observations, under perfectly natural conditions, that frogs in cold weather (so-called 'winter frogs'), in water at 0° to 15° C., remain with their heads above the surface from one-tenth to one-half the time, and while above the surface carry on from eight to twenty lung respirations per minute; showing, that, under natural conditions, the respiration of 'winter frogs' is not entirely or almost entirely carried on aquatically by the skin, as is commonly supposed (Klug and Martin).

4. The results obtained by Moreau and others, upon the respiratory function of the air-bladder of ordinary fishes, and those of Wilder, on the respiration of Amia (the mud-fish), are in general accord with the facts stated for turtles and tadpoles.

These facts seem to us to justify the conclusion that the respiratory gas-interchange in combined aerial and aquatic respiration does not conform to the law governing either exclusively aerial or exclusively aquatic respiration, but that, whenever aerial and aquatic respirations are combined in an animal, the aerial part of the respiration is principally to supply oxygen, and the aquatic part to get rid of carbon dioxide. S. H. and S. P. GAGE.

Anat. lab., Cornell univ.,
April 15.

Pharyngeal respiratory movements of adult amphibia under water.

In studying adult amphibia for possible respiratory movements under water, we have found that the common newt (Diemictylus viridescens) so abundant in lakes and ponds, and which is known to remain voluntarily a long time under water, carries on, while under water, rhythmical pharyngeal movements almost precisely like those of the soft-shelled turtles; and, as in the turtles, these movements cause a flow of water into and out of the mouth and pharynx.

The Cryptobranchus (Menopoma) has also been found to draw water into the mouth, and to expel it, in part at least, through the persistent gill-fissures.

So far as we know, these facts have not been published before. We would be glad to know if these observations have been previously made on Diemictylus and Cryptobranchus, and if similar pharyngeal movements under water have been described for other adult amphibia. S. H. and S. P. GAGE.

Anat. lab., Cornell univ., April 25.

The germination of pond-lily seeds.

In the issue of *Science*, March 21, 1884, there appeared a conditional offer of seeds of the Nymphea odorata, obtained by me in the fall of 1883, the growth of that year. Many of the seeds at this time were germinating; some had developed the second leaf. There was a marked difference in color; the variations were, in shades of red, from blood-red to light pink, from dark blue-green to light yellow-green, and from a dark bronze to a light salmon. It seemed to me, with varying and suitable culture, new varieties might be obtained, as the seeds are not always to be had, and the method of germination is not a matter of every-day observation. A number of applications were received, but I have not heard from any one, of successful culture, nor whether all or any of the seeds germinated. A succession of germinations gave me new plants to take the place of those destroyed by Unios, ferments, or fungi. The seed were kept under water, on sand, exposed to a north light, or that reflected from the brick houses on the north side of the street, fifty feet distant.

In June, 1855, I removed from the water all light seed, and those that were softened, as well as all on which fungoid growths had appeared, and placed the vessel in an open space where it had vertical light, and from the sun, for an hour between eleven and twelve in the morning in clear weather. A half-dozen new plants appeared in August, as the result of the change. When the cold weather came in the fall, I restored them to their old position in the north light, slightly obscured by ferns, Zygodium scandens and Pteris serrulata. About last Christmas I observed a new plant that had germinated since being brought in in the fall. This plant was removed to some submerged soil in another vessel, where it is now putting forth its fourth leaf. In February another seed germinated; and, since the 20th of March, three others have begun to grow. The last one was observed on the 3d of April. There are a few more very heavy seed in the water. The first plants from these seed that germinated early in 1884 — beginning in January — were peculiar in the length of the internodes, all being very long, some over an inch; and the seeds, before germination, were very light, and quite variable in color, but not as much so as the foliage.

The germinations of 1885 have shorter internodes, smaller leaves, of an even green color, whilst other germinations of this year have the internode reduced to a minimum; the leaves seem to start from the very dense and dark seed; and the foliage is variable in size and color, but mostly in light shades of bronze — salmon — with shades of pink.

The seeds varied in their development when taken from the pond in which they grew.

Some of the plants had just begun to coil the flower-stem by which to draw the seed down to the bottom of the pond; one had finished coiling, and the seed-vessel was in the mud; others were midway between these extremes. I mention this to show that there were natural and well-known causes for the variance in time of germination.

When it is known that the ripe and fully matured seeds are very dense, it will not seem so strange, that, considering the great number of seeds to a single flower, all ponds are not overcrowded, as by their density they sink into the ooze and remain dormant.

I shall note with interest any future germinations as lengthening the possible dormant period of these seed.

On April 19 I observed five more germinations, with the characteristics of those mentioned as growing this year. Up to April 24, three other young plants had started, making thirteen since Christmas; and these are as vigorous as those that started in 1884, — much more so than the growth of the summer of 1885.

GEO. F. WATERS.

8 Beacon Street, Boston, Mass., April 24.

Eskimo building-snow.

In *Science* for April 23, 1886 (p. 372), Sergt. T. W. Sherwood has an inquiry about a certain formation of snow. I refer you to a paragraph in *Science* for April 25, 1884, p. 822, concerning 'ice-banners,' from observations of my own.

GILBERT THOMPSON.

U. S. geol. surv., April 23.

Certain homologous muscles.

The writer, having devoted some time of late to a comparative study of the myology of American mammals, has noted several interesting facts, to one of which attention is here asked.

The myology of the shoulder is, perhaps, more interesting than that of any other region, inasmuch as the variations in structure can usually be readily correlated with corresponding variations in habit. This is true in particular when applied to those changes observed in members of the same genus and family. In a forthcoming work I hope to present a mass of details illustrating the nature of these variations.

The muscular system is so plastic, and so immediate an expression of function, that it was hardly expected that many hints bearing on phylogeny could be derived from that source. Osteology, possessing as it does so many advantages in this respect, has been trusted far too exclusively, as I hope to show: at least, a careful study of the anatomy of the soft parts may be expected to furnish much confirmatory evidence. In the case of the shoulder, the omo-hyoid muscle may be said to furnish a valuable criterion by which to determine the primitive character of a species. Its presence in the archaic types, and frequent absence in specialized forms, can hardly be correlated with change in function.

The sciurimorphs are a very compact group, and yet present a great variety of modifications in adaptation to variation in habit. Among the members of the group found in the United States, the woodchuck (Arctomys monax) is perhaps entitled to rank as the most primitive form. This conception is suggested by the osseous structure, and finds an interesting support in a number of points in the myology, only two of which are here mentioned. The omo-hyoid passing from the sterno-hyoid to the anterior margin of the clavicle is very well developed. A very important part of the skin-muscle forming the covering of the cheek is derived from a broad, flat band springing from the anterior third of the sternum, the insertion being in the skin of the lips and chin. But most curious of all is the presence of a well-developed skin-muscle springing from the lower posterior free margin of the rhomboideus dorsalis, which, unlike the cucullarius, has an origin far down the back, overlapping the latissimus. The thin band of which mention is made is entirely distinct from any portion of the paniculus until it reaches the region of the cheek, where its fibres appear to lose themselves upon the skin. What gives these points interest is the fact that the only other rodent yet encountered, which has such a muscle, is Geomys, the pouched gopher. In G. bursarius an exactly similar muscle springs from the latissimus at almost the identical point, and has exactly the same course, its insertion being on the pouch, whence I have elsewhere termed it retractor bursae.

In none of the myomorphs examined has such a muscle been encountered. Without going into further detail, it will be sufficient to point out the fact that there may here be a hint of the antiquity, if not consanguinity, of these types, unless, indeed, it can be shown that an underground habit has developed in one case, — that which has its apparent explanation in the function dependent on the possession of a pouch in the other.

In the chipmunk, which is pouched, though only imperfectly fossorial and more perfectly sciurine, this muscle is absent. The spermophiles, although the nearest living American allies of Arctomys, do not possess this muscle. In the flying squirrel there is a thin band of muscle passing from the wrist, having its origin on the carpus opposite the volar spur, and passing to the same point as the muscle here described. The flying-squirrel also has a distinct omo-hyoid.

C. L. HERRICK.

Dennison university, April 12.

A means of distinguishing the Canada lynx from the Bay lynx.

If a dozen zoölogists were asked how many species of lynx exist, the majority would probably decline to commit themselves to any opinion, while among the rest would be found advocates for a varying number of species, — as few as one, perhaps, or as many as eight or nine.

While examining a series of sixty or seventy skulls of American lynxes recently, I hit upon two characters which will, I believe, prove useful in distinguishing between the species more satisfactorily than has been possible hitherto. I found that in all the skulls from far north, indeed in all that were labelled 'L. canadensis,' the anterior condyloid foramen is large, looks downward, and is not confluent with the foramen lacerum posterium; and that the visible portion of the presphenoid is flask-shaped, the convexity being in front. In all the skulls of L. rufus, maculatus, and fasciatus, on the contrary, the two foramina are confluent, as in the cats generally, and the visible portion of the presphenoid is sagittate or linear.

The single skull of Lynx borealis in the national collection, and one of L. cervaria, exhibit the characteristics of L. canadensis.

It would appear that in the case of the American lynxes we are dealing with two distinct species only: 1°, L. canadensis; and, 2°, L. rufus, with its varieties fasciatus and maculatus. It is also probable that the confluence of the condyloid and lacerated foramina cannot hereafter be regarded as a distinguishing character of the Aeluroidea.

FREDERICK TRUE.

Washington, April 20.